

## CRITHAGRA RENDALLI, sp. nov.

C. ♂. Fronte et loris flavis : capite et collo flavis, brunneo angustè striatis : scapularibus, tergo, et uropygio brunneo-nigris, flavo-marginatis : subtùs totus aurantiaco-flavus : remigibus et rectricibus fuscis, flavo-marginatis : subalaribus pallidè flavescentibus : rostro nigro : tarsis et pedibus fuscis.

♀. Suprà nigrescenti-fusca, plumis albedo-marginatis : loris et spatio interoculari albidis : subtùs pallidè flavida, striis fuscis rarioribus angustioribus : subalaribus albescentibus : rostro, tarsis, et pedibus fuscis.

Long, tot. 5 poll., alæ 2·75, tarsi ·66, rostri à rictu ·5.

*Hab.* Barberton, Transvaal.

Both specimens were taken on the 16th Feb., 1894.  
[“ Irides hazel,” P. Rendall.]

X.—*On Use and Abuse of Generic Terms.* By H. B. TRISTRAM,  
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I PRESUME it will be universally admitted that no part of the scheme of the great founder of modern natural science is of greater practical value to the student than the binomial system—or, inasmuch as genera are purely ideal, I might more accurately say, the binomial theory of Linnæus. Yet, though genera are arbitrary divisions, and therefore can be multiplied or diminished at pleasure, it surely does not follow that any author is justified in capriciously adding to their number.

A genus has been defined to be a re-union of races called species, brought together by a consideration of their relations, and constituting so many small series, limited by characters which are chosen arbitrarily in order to circumscribe them. But, though chosen arbitrarily, they should surely be chosen consistently. Forgetfulness of this principle by writers each of whom has his own idea, or more frequently none at all, of the conception of a genus, has led to the repulsive list of synonyms in barbarous and grotesque compounds, purporting to be derived from the classic tongues, which follow the selected generic name in every ornithological treatise we open.

Now, genera being arbitrary arrangements, invented simply for convenience, surely there should be some general principles agreed upon to check their needless multiplication—*i.e.*, some recognized rules as to structural differences. If a genus be a group of species, having one or more characters in common in which they resemble each other, and having one or more common characters in which they differ from all other species, we may set a limit to the confusion produced by the ever-multiplying lists of genera; for, as Linnæus has reminded us, “confusis nominibus, omnia confundi necesse est.” Genera being invented for convenience, convenience should be kept in view in their use. I well remember the late Lord Tweeddale remarking that we ought never to invent a genus, unless for a clearly defined *structural* difference, except in cases where the number of known species is inconveniently large, and then, as genera are after all arbitrary, we are fully justified in dividing the genus. Lord Tweeddale instanced the case of *Turdus*, which would be overwhelmingly numerous unless *Merula* were separated from it. But if we had known only three or four species of each, he would not have separated them. So with Linnæus’s genus *Motacilla*. If that had been retained unbroken, the binomial system might as well never have been invented.

The question is—Is the multiplication of genera each containing one or two species, and those closely allied, an aid or a hindrance to the study of the subject? To quote the words of Dr. Sharpe on another point in nomenclature, and which I would apply to many of the new-fangled genera:—“An arrangement we shall never adopt, as we consider it a clumsy and unnecessary method of nomenclature, and one that in the hands of unscrupulous writers may be employed *ad lib.* to gain a little temporary notoriety, and end in making the study of birds impossible. Can any science bear the weight of such a system of nomenclature?”

In this matter of the multiplication of genera, the practice of different authorities presents startling contrasts. Let us take 20 volumes of the British Museum Catalogues, consisting wholly or partially of Old-World birds. Mr. Seebohm

compiled one volume, Dr. Gadow two. Neither of these invented a single new genus. Mr. Salvin in his volume introduced one new genus; Mr. Hargitt four; Capt. Shelley five, four of which he had previously published; Mr. Ogilvie Grant, in a volume and a half, six new genera; Count Salvadori, in the volumes on the Parrots and Pigeons, 12 genera, of which he had already published 5; while Dr. Sharpe, in 10½ volumes, has favoured us with 108 new genera. It is obvious that the "genus-standard" of Dr. Sharpe must be very different from that of Messrs. Hargitt, Seebohm, Salvadori, and others, who in 9½ volumes have been content with 28 new genera, as against his 108.

Dr. Sharpe has certainly made some genera that will stand the test of time. No one can carp at *Chytoceyx* as not being a good genus; but at least 100 of his generic diagnoses would have been treated as simply specific by his collaborators in the B. M. Catalogues, if we may judge by their own generic definitions. My complaint of this "genus-facture" is that it is absolutely capricious; that the authors seem to be guided by no settled principles; that it overloads us with synonyms; and that, so far from being a help, it is an actual hindrance to the student. In fact, it is doing for genera what Brehm did for species; and as most writers ignore his species, so, in mercy, it is to be hoped that many of these genera will be consigned to oblivion. Among the prominent offenders are several of our German friends and our American cousins, but of native authors certainly Dr. Sharpe is pre-eminent; and I fear he does not improve with age, for his last volume, with its 18 new genera, 16 of which comprise but 17 species, surpasses all its predecessors. There are indisputable genera in the same volume which comprise but one or two species, but what analogy is there between such genera as *Notornis*, *Tribonyx*, or *Pennula*, and *Amaurolimnas*, *Limnogeranus*, and *Sarcogeranus*? or, to take an earlier example, *Rhinocorax*—a true Raven, if ever there was one, but transferred to generic solitude, because its upper nasal bristles have an upward turn? I can only say that if genera are to be so multiplied we shall soon be little better off than

before Linnæus struck out his binomial system. In Dr. Sharpe's words "It will end in making the study of birds impossible"; and I do implore him in future volumes to study, not foreign examples, but those of his own colleagues in that great series of which we owe so large a part to his own laborious energy.

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XI.—*Further Remarks on the Mode of Carriage of the Legs in the Birds of Prey.* By ERNST HARTERT.

IN 'The Ibis' for 1894 (p. 547) the Editor called attention to my remarks about the carriage of the legs by the Raptores, and asked the Members of the B.O.U. whether I was right or not. I hope they will publish their observations, as there seem to be different opinions. At the same time such questions can certainly not be settled by opinions, but only by accurate observations.

I myself was formerly under the impression that the Raptores carry their feet drawn up in front against the abdomen, because all the figures and all the stuffed birds which I have seen were arranged thus. I was therefore much astonished to find that *Milvus govinda* and *Haliastur indus*, which are so fearless in India that one can observe them quite closely, carried their legs stretched out behind under the root of the tail, and afterwards I saw the same course followed by birds of several other species. I therefrom conclude most positively that all the Raptores do the same, for such habits as this are never peculiar to certain species. Besides, a number of exact observers, both in Germany and Africa, have published in the 'Ornithologische Monatsberichte' their own observations on other species of Raptores (after I had opened the controversy), which agree with my own. It is true that the contrary has been stated by two observers, but they make such sweeping and general statements that I do not doubt that their memory failed them. They evidently wrote from recollection, and did not quote particular observations of the fact.